Some comments on those chapters in Kerner and Oliver's "Natural History of Plants," which deal with reproduction.

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The bringing out of an American edition of Kerner and Oliver's *Natural History of Plants*, together with its great attractiveness and generally great value, makes it certain that this work will be used throughout the United States as a reference book or encyclopedia of botany. It therefore seems sufficiently worth while to give some attention to the ideas that are promulgated in its pages. It is not a particularly pleasant task to point out imperfections in so beautifully printed and skilfully compiled a work as the one in hand, but if botanical students are to be referred to this *Natural History of Plants* by their teachers, and it is to be held before them as an authority, it is of the greatest importance that some of its shortcomings should be known that they may be guarded against by teacher and by pupil.

I shall confine myself in this paper to indicating some of the errors, as I take them, in only one division of the work—that is, the chapters on the *Genesis of plant offspring*, in volume II, pt. 1 (half-vol. III of the four vol. edition, Henry Holt & Co.). It is not too much to say that this part of the Natural History is absolutely untrustworthy, not only in its statements of theory but again and again in its statements of fact. I have convinced myself by reference to the original German edition that these errors are not those of the translators. In order to point out a few of them a series of quotations and comments will be given.

p. 6. "In most—probably in all—divisions of the vegetable kingdom, two kinds of propagation occur. In each case a single protoplast forms the starting point for the new individual but in the one, this protoplast does not require the special stimulus afforded by union with another."

p. 46. "If a fruit is to arise, the ooplasm, i. e. the protoplasm destined to initiate a new generation, must unite with the fertilizing protoplasm which is called spermatoplasm," and p. 46, "the union of two protoplasts constitutes the essence of fertilization."

Comment. The last quotation is truth but at variance with

the others and, as will be shown, deeply opposed to later statements. It is not true that the "single protoplast" which forms the starting point for a new individual, sexually produced, requires any such stimulus. *This* "single protoplast" is the syngamete or resultant cell from gametic fusion. It is also erroneous to suggest that from the ooplasm arises the new individual. This, on the contrary, arises from the syngamete.

p. 9. "As the spores of ferns are not the direct result of a process of fertilization they are not parts of fruits but brood-bodies."

p. 15. "The only structure rightly to be considered a moss-fruit is that in which the embryo is produced as a result of fertilization."

p. 16. (Describing rupture of moss-calyptra.) "The coat of the *fruit* being torn away."

p. 47. "The ooplasm rendered capable of fertilization, of this particular kind of growth" (i. e., into a new generation) "is to be considered as an embryo, even in cases where no visible change has taken place."

p. 66. In mosses "it is best to look upon the formation of fruit as being complete as soon as fertilization has taken place."

Comment. Clearly the word *embryo* is used here as a synonym of fecundated-egg, oosperm, or syngamete. The structure called a moss-fruit is, as clearly, a fecundated egg together with the enclosing archegone. And the further development of a moss sporophyte is called a development from the *fruit*. Here terms are used in an unusual sense, but not even consistently as the context will show.

p. 47. "We consider every structure to be a fruit which is the product of fertilization and at the same time constitutes the first step towards the renewal of the fertilized plant."

Comment. From the above it is clear that the only structures properly termed *fruits* in flowering plants would be the micropylar syngamete nucleus of the embryo-sac (after fertilization has taken place), or the antipodal syngamete nucleus (under the theory of Morot, that this cell represents gametic components). Now as a matter of fact, it is not these structures that are termed fruits at all, by Kerner, but those entirely different bodies—the fruits in the popular sense. See p. 48.

"At one end of the chain we have the unicellular fruits of the microscopic desmids, at the other the fruit of the cocoa-nut, which is differentiated into seeds on the one hand, and, several envelopes on the other and is as large as a man's head."

p. 49. Cryptogams possess "organs of fructification not clearly visible without aid from the microscope, whilst the term Phanerogam will comprise such plants as have organs of fructification which are visible without aid from the microscope."

Comment. Remembering the definition given of *fruit* and of *fertilization*, it is at once apparent that the word fructification is used ambiguously above. Under Kerner's own definition the *fruit*, that is the body or embryo, arising from the fusion of two protoplasts, is quite as invisible to ordinary eyes in phanerogams as in cryptogams. Nor are the "organs" any plainer in the one case than in the other.

p. 49. "In cryptogams fertilization takes place in water or in a watery medium, whereas the process in phanerogams is accomplished almost exclusively in the air."

Comment. This is the old confusion between pollination, which is the scattering of spores on a favorable locality, and fertilization which, as is properly stated by Kerner, consists in the union of two protoplasts. As a matter of fact fertilization in phanerogams, under Kerner's own definition never takes place "in the air," but always in the tissues of the ovule and ovary. It would be quite as proper to say that the fertilization of all vascular cryptogams takes place "in the air," since in these spores are blown out of the sporangia into the atmosphere, thence to light on some favorable germination spot.

p. 47. "The cell wherein the spermatoplasm is brought to the proper form and composition for the purpose of fertilization is called an *antheridium* in the case of a cryptogam, and a pollen-grain in the case of a phanerogam."

p. 85. "Pollen consists of cells which contain spermatoplasm, and may be compared to the antheridia of cryptogams."

Comment. A more thoroughly vicious statement does not exist in plant morphology than this. Almost every state ment and inference in it is erroneous. For the term antheridium, at least among archegoniates, is employed to designate, not a cell but an organ, and is properly employed by Kerner farther on (p. 65), where he describes in the ordinary manner how moss antheridia are produced, "mingled with paraphyses." The "cell in which the spermatoplasm is prepared," etc., is a *sperm-mother-cell* or spermatocyte, not an antheridium. Nor is a spermatocyte or antheridium in any degree homologous or analogous with a pollen-grain. For this, as every one knows, is a spore and produces a plant one cell-nucleus of which is a sperm, nor is there any definite antheridium or spermocytes in metaspermic flowering plants.

p. 68. Discussing the Filices: "The fertilized ooplasm now subdivides . . . and thus is produced a multicellular embryo which remains imbedded in the unaltered amphigonium (archegone). This

structure, though scarcely differing at all from the fruit rudiment must be considered as a fruit. After a short period of rest the embryo germinates and the new generation which gradually makes its appearance as stem, roots and fronds emerging from the embryo continues to receive its food-stuffs through the mediation of the prothallium."

Comment. Here is an amazing account of the regular development, from the syngamete, of the ordinary sporophytic fern. The conception of the spherical embryo "germinating" is peculiarly gratuitous, nor is there the dormant period referred to. One might as well speak of the babe "germinating" after a dormant period and becoming a man.

p. 69. The account of the Rhizocarpeæ and Selaginellaceæ is badly confused. For example, speaking of the germination of microspores it is said that in Salvinia, Marsilia and Selaginella one or two cells are "pushed out through rents made here and there," whereas as a matter of fact this does not occur in any of the genera mentioned except *Salvinia*, nor is the phrase "rents here and there" at all definite enough.

p. 69. "The tissue produced from a macrospore in the Rhizocarpeæ and Selaginelleæ has been compared to the ovule as it occurs in the phanerogams."

And after a few comments on this surprising alleged homology, Kerner adds:

"But if it is made the basis of far-reaching speculations concerning the evolution of one group of plants from another, the descent of phanerogams from cryptogams, for example, I must enter an emphatic protest against any such proceeding."

Comment. The emphatic protest might have some weight if any such homology had ever been suggested. As a matter of fact the tissue produced from a macrospore has no possible homology with an ovule and no informed botanist since the days of Hofmeister ever supposed that it had. The macrosporangium of Selaginella with its four contained macrospores is maintained to be a homologue of the ovule of Rosa or of Casuarina, and with very good reason too. And a whole sorus in Marsilia including both kinds of sporangia or a megasorus of Salvinia or still more closely a unisporangiate megasorus in Azolla might be maintained as homologous with an ovule. Whoever heard of any botanist homologizing the female prothallium of Selaginella with an ovule? This structure is often homologized with the endosperm of the coniferous or cycadeous seed and I think with propriety, but never in all my reading have I heard of its being homologized with an ovule. Such setting up and solemnly knocking down of mor-

phological straw-men is a reprehensible practice. It becomes doubly so when a writer after knocking down his unrecognizable dummy tells us that it bore the theory of evolution about its garments.

p. 84. "The nucellus of the ovule arises in many instances (*e. g.* in orchids) from a mass of tissue produced by the division of a single epidermal cell." This is based on Hofmeister's statement, but I believe it is contradicted by later research.

p. 401. "Pollination is only the prelude to the phenomenon known as fertilization. It is important to distinguish clearly between these two events."

Comment. Here the proper view of pollination is presented, but no withdrawal of the statement that flowering plants are "air-fertilized" while flowerless plants are "water-fertilized." Indeed (p. 71, bottom and 72, top) it is expressly stated that the reason cryptogams lack blossoms is because these are not needed for aquatic fertilization, while they are for air-fertilization, hence are developed by flowering plants.

In general the pages 401–427 in which the true fertilization, or, better, fecundation, of the metaspermic egg is considered do not connect with the earlier chapters. This is due to the careful rewriting of the latter part, by the editor, I presume. At any rate it reads differently enough from the German original where the same mixture of terms goes on from cover to cover.

There are many more of these errors and confusions in the third half volume of the *Natural History of Plants.* I have not time to point them out but may if it seems necessary contribute a series of comments like the above upon other points that might prove dangerous if not turned in the right direction. In general I am compelled to say, after a careful and complete perusal of the *Natural History of Plants*, that while as a popular store-house of botanical facts it is indeed a mine of information to the one who knows gold from pyrites, it is quite unsafe to consider all that glitters, gold. There are a large number of facts in it which "are not so." And second, as an expression of botanical theory I consider it generally sound but here and there insidiously and insistently misleading. To the trained student of botanical science these slips will not prove troublesome but to the less widely informed reader they will be dangerous.

To sum it all up: the work is invaluable to the thoroughly informed botanical teacher or investigator; he can use the

good and discard the bad. It is equally to be commended to the general reader who will profit by what is true and excellent and will not be particularly harmed by the faults. There is one class, however, to whom this book would be an almost unmixed evil and that is to the group of young men intending to become professional botanists. If they base their botanical information or speculation upon the *Natural History of Plants* they will in several important phases of the science find themselves badly confused and misinformed.

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